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EXPERIMENTS AND OBSERVATIONS
RELATING TO THE ANALYSIS OF
ATMOSPHERICAL AIR;

ALSO,
FARTHER EXPERIMENTS
RELATING TO THE
GENERATION OF AIR FROM WATER.

READ BEFORE THE AMERICAN PHILOSOPHICAL
SOCIETY, FEB. 5 AND 19, 1796; AND
PRINTED IN THEIR TRANSACTIONS.

TO WHICH ARE ADDED,
CONSIDERATIONS ON THE
DOCTRINE OF PHLOGISTON,
AND THE
DECOMPOSITION OF WATER.

ADDRESSED TO
MESSRS. BERTHOLLET, &c.

By JOSEPH PRIESTLEY, L. L. D. F. R. S. &c. &c.

PHILADELPHIA, PRINTED.
LONDON,
REPRINTED FOR J. JOHNSON, IN ST. PAUL'S CHURCH-
YARD.

1796.

EXPERIMENTS AND OBSERVATIONS
RELATIVE TO THE ANALYSIS OF
ATMOSPHERIC AIR

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PHILADELPHIA, PAINTED

LONDON

PRINTED FOR J. JOHNSON, IN ST. PATRICK'S CHURCH-YARD

1864

EXPERIMENTS AND OBSERVATIONS

RELATING TO THE

ANALYSIS

OF

ATMOSPHERICAL AIR.

IT is an essential part of the antiphlogistic theory, that in all the cases of what I have called the *phlogification of air*, there is simply an absorption of the dephlogisticated air, or, as the advocates of that theory term it, the *oxygen* contained in it, leaving the phlogisticated part, which they call *azote*, as it originally existed in the atmosphere. Also, according to the principles of this system, *azote* is a simple substance, at least not hitherto analyzed into any other: They therefore suppose that there is a determinate proportion between the quantities of oxygen and azote in every portion of atmospherical air, and that all that has hitherto been done has been to separate them from one another. This proportion they state to be twenty-seven parts of

oxygen and 73 of azote, in 100 of atmospheric air.

But in every case of the diminution of atmospheric air in which this is the result, there appears to me to be something emitted from the substance, which the antiphlogistians suppose to act by simple absorption, and therefore that it is more probable that there is some substance, and the same that has been called phlogiston, or the *principle of inflammability* (being common to all bodies capable of combustion, and transferable from any one of them to any other) emitted, and that this phlogiston uniting with part of the dephlogisticated air forms with it part of the phlogisticated air which is found after the process; and in some cases there is more of this, and in others less. Also, in some cases, fixed air is the result of the union of the same constituent principles.

A mixture of iron filings and sulphur, which, with a little water, has been commonly made use of to diminish and phlogisticate air, and probably many other substances which produce the same effect, if they be continued in the air after the diminution has advanced to its *maximum*, occasion an increase of the quantity, by an addition of inflammable air. This mixture I find to have the same effect when it is long confined in nitrous air, or in fixed air; and therefore it is probable that

that the same would be the case if it were confined in any other kind of air, or in vacuo. It, therefore, seems natural to infer, that the same principle which constitutes inflammable air was from the first exhaling from the mixture, but that it did not actually form inflammable air till there was no more dephlogisticated air for it to unite with, and thereby form phlogisticated air. The experiments from which this conclusion is drawn are recited in my former publications, and I have lately repeated them with particular attention, and the same result. I have also lately observed that by heating bones made black by burning without access of air, in atmospheric air, there is, after the period of greatest diminution, an increase of the quantity, and that it is then found to contain a mixture of inflammable air.

That something is really emitted from the iron filings and sulphur, when it is in a state of diminishing air, is evident from the strong and offensive *smell* which at that time this mixture has. Flowers also, and especially those which have the strongest smell, I have observed to phlogisticate air. Moreover, the iron filings and sulphur when nearly dry, emit a visible dense vapour, which appears by its smell to be vitriolic acid air, which I have observed to have the power of diminishing and phlogisticating air; owing, no doubt, in part, to its imbibing the dephlogis-

ticated part of it, and with it forming common vitriolic acid; but at the same time part of its phlogiston may unite with another part of the dephlogistified air, and with it form phlogistified air.

Iron filings and sulphur, as well as phosphorus, and most of the other substances which have been generally used for the purpose of phlogistifying atmospherical air, do likewise imbibe the dephlogistified air contained in it, and thereby gain an addition of as much weight as the air has lost. But this is not the case with *black bones* heated in air, which by this means become white; and as nothing in them is volatile, except that which constitutes their blackness, I thought they would be a very convenient substance with which to make these experiments.

These bones gained no addition of weight in the process, and when they are used, the diminution of the air is by no means so great as in the other cases, though the air that is left is completely phlogistified. This is probably in consequence of the fixed air (formed by the union of the dephlogistified air with the phlogiston emitted from the bones) not being readily imbibed by the water, or any other substance with which it is then in contact; so that a better opportunity is given to the phlogiston emitted from the bones to unite with that air in a different manner, and form phlogistified air, which
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is therefore after the process found in a greater proportion than in the other cases, to which alone a due attention has hitherto been given. When these bones are heated over lime water, there is a copious precipitation of the lime. Here I would observe, that the phlogiston necessary to form this fixed air could only come from the bones in becoming white, as they had been calcined in as great a degree of heat as I could produce, so that no kind of air could have been expelled from them while excluded from access of air.

Having by means of a burning lens heated 140.5 grains of well burned black bones in 23.75 ounce measures of air, it was reduced to 20 ounce measures, completely phlogistified, without any mixture of fixed or inflammable air in it. According to this experiment, the quantity of pure air in 100 ounce measures of atmospheric air was only 15.78 parts instead of 27.

Heating 267 grains of these bones in 30 ounce measures of air, it was reduced to 25.5 ounce measures completely phlogistified, which was in the proportion of 15 parts of dephlogistified air in 100 of atmospheric. In these experiments with bones there is sometimes a small loss of weight, owing, I doubt not, to something besides phlogiston being expelled from them by the intense heat of the lens; and during the

process I could perceive a slight vapour rising from them. When I managed the heat so that it was not more than necessary to whiten the bones, they neither gained nor lost any weight; at least the loss was very inconsiderable.

I had similar results from experiments made with small polished *steel needles*. For when they were heated so as only to become blue, and were not melted, they gained very little, if any, weight, and diminished the air only in about the same proportion with the black bones.

Having by means of a burning lens heated 200 grains of the polished needles in 24 ounce measures of air (in consequence of which they became of a dark colour) they neither gained nor lost any weight, and the air was reduced to 19.5 ounce measures, almost completely phlogisticated. I heated the same quantity of these needles in 16.75 ounce measures of air, when it was reduced to 13.5 ounce measures, completely phlogisticated without any mixture of fixed or inflammable air in it; so that the diminution was in the proportion of 19.4 parts in one hundred. In another experiment 24.75 ounce measures of air were reduced to 20.25 ounce measures nearly phlogisticated. It is evident, therefore, from these experiments, that more phlogisticated air is found after this process, than after that with the iron filings and sulphur.

Because by heating the needles over a quantity
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of water they might attract moisture, which cannot be expelled from them without some difficulty, I heated 200 grains of the same needles in the open air, till they had assumed exactly the same appearance with those that had diminished the air in the preceding experiments, and found that they had neither gained nor lost any sensible weight. The same was the result of whitening a quantity of black bones in the open air. But to make this experiment with accuracy, the bones should be calcined with the greatest degree of heat, and made white with the least.

In one experiment with very thin pieces of malleable iron (viz. iron turnings) 38.5 ounce measures of air were reduced to 31.5 measures, wholly phlogisticated, which is in the proportion of the loss of 19.5 parts in 100. I could not perceive that the iron had gained or lost any weight; whereas, if it had imbibed the air that had disappeared, or the water, of which, as I have shewn, the air principally consists (as it would have done if it had been melted in the process) it ought to have gained 4.2 grains.

There was not, however, the same certainty in the experiments with the needles, and still less with the iron, as in those with the bones. They generally gained a little weight, and diminished the air more than the bones. The reason of this uncertainty might be, that they were

were sometimes heated too much; and sometimes fine scales were thrown from them, which were indeed sometimes visible when, in floating about within the vessel, they crossed the sun beams, and both in the experiments with the needles and those with the bones a vapour visibly rose from them. When the needles were heated over lime water, a thick crust was formed upon it; but there was not such a precipitation of the lime as in the experiments with the bones.

That the phlogification of nitrous acid is owing, in some cases, to its *imbibing* something, and not always to its *parting* with any thing, which the antiphlogistians maintain, is evident from its becoming phlogificated by imbibing nitrous air. This I have observed that it does with the greatest rapidity, leaving in some cases not more than one 18th part of the original quantity. M. Fourcroy supposes (*Philosophie Chymique*, p. 76) that the conversion of the common nitrous acid into the phlogificated is always occasioned by its parting with oxygen. That this is sometimes the case I have demonstrated in my experiments with heating it in long glass tubes; but in the present case it is not possible that the acid should have parted with any thing, and least of all with oxygen, since the small residuum of nitrous air is pure *azote*. I shall here observe, what I did not before, that the

the absorption of nitrous air by nitrous acid is attended with a considerable degree of heat.

That phlogisticated air, or azote, is not a simple substance, but consists of phlogiston (or whatever is the proper element of inflammable air) and of dephlogisticated air, is probable from several experiments that seem to have been overlooked by the antiphlogistians, such as the following. A mixture of dephlogisticated and inflammable air being kept a long time, was found by M. Metherie to contain a considerable portion of phlogisticated air, as appeared by the difference of the residuums after exploding a part of the mixture when first made, and another part some time afterwards. I had also found that a mixture of dephlogisticated and inflammable air suffers a considerable diminution in a course of time, though they will not wholly incorporate. But I have lately found that these two kinds of air unite completely by being confined some time together in a moist bladder.

Having mixed equal quantities of those kinds of air, I put them into a bladder, which I left floating in a trough of water, and found, after about a fortnight, that the quantity was considerably diminished; and examining it, I found it to be almost wholly phlogisticated, though there was something slightly inflammable in it. On this I put equal measures (but omitted to note the quantity) of each of the kinds of air into another

another bladder, and after about three weeks, found it reduced to 12.5 ounce measures, all pure phlogisticated air, without any mixture of fixed or inflammable air that I could perceive.

I have likewise hit upon another method of forming phlogisticated air by the union of dephlogisticated and inflammable air, viz. by exposing the latter to a surface of rusted iron, which is known to become so by imbibing pure air. Twenty ounce measures of inflammable air were confined in a phial containing pieces of rusted iron from the 18th of August to the 6th of October, when it was reduced to 9 ounce measures, and was but slightly inflammable. The iron, from being red, was then become of a very dark colour. Another quantity of inflammable air treated in the same manner from, I believe, the 6th of October, was in the 2d of December found to be completely phlogisticated. In these experiments the iron and the air were confined by water. Afterwards, putting 7 ounce measures of inflammable air to pieces of rusted iron confined by mercury, it was, in about a week, almost wholly absorbed. I then filled up the vessel again with inflammable air, and when the diminution proceeded no farther, I examined it, and found 5 ounce measures of air completely phlogisticated.

Charcoal, as well as phlogisticated air, I have no doubt, contains the element of dephlogisticated

cated air, as well as phlogiston, since by its union with steam it takes the form of fixed air, as well as that of inflammable air, and one element in the composition of fixed air is dephlogisticated air. And when I made hot charcoal imbibe inflammable air by introducing pieces of it into jars of this air confined by mercury, and afterwards expelled it by plunging the charcoal in water, that which came out of it was phlogisticated air. Yet I think I recollect that the result of this experiment has sometimes been inflammable air, the same that the charcoal had imbibed.

I know of no case of the simple absorption of air, but which, like that by water, respects all kinds of air, though with a preference of that which is dephlogisticated; but not so as to take this kind *only*, and leave all the phlogisticated air that was mixed with it. Otherwise it would have been in our power to ascertain with exactness the real proportion of both the kinds of air in the atmosphere. For want of this the nearest approximation that we can make appears to me to be by the use of nitrous air.

Since when two measures of pure nitrous air are mixed with one measure of pure dephlogisticated air, they both, as nearly as possible, disappear, and form nitrous acid, which is imbibed by the water in which the mixture is made, it is evident that little or no phlogisticated air is

necessarily formed in this process; and when it is conducted properly, there will appear to be a much greater proportion of dephlogisticated air in the atmosphere than has been supposed, and enough to be converted into phlogisticated air in the process above mentioned. But a considerable *time* is necessary for this purpose; because the diminution continues much longer than has been hitherto imagined.

The difference between the degree of diminution of atmospherical air by a mixture of nitrous air, with, or without, *agitation*, is very considerable. In general, without agitation, equal measures of each will occupy the space of 1.25 measures, but with agitation only 1.01; and if the computation be made from this last *datum*, it will give the proportion of dephlogisticated air to be 27 parts in 100, and consequently that of the phlogisticated air 73. But by keeping the mixture a longer time, the diminution will proceed to about 0.6 of a measure, which will give 46.6 for the proportion of dephlogisticated air, and 53.4 for that of the phlogisticated air in the atmosphere.

This diminution in the mixture of nitrous and atmospherical air, which is effected in the course of time, is various, depending, no doubt, on several circumstances which I have not yet been able to ascertain. What I have actually observed is as follows;

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On the 21st of July I mixed equal quantities of nitrous and atmospheric air; when, with agitation, they occupied the space of 1.01. Examining the mixture at different times, I observed that the diminution kept advancing till some time before 24th of August, when it occupied the space of only 0.545. Another mixture made in the same manner was 0.54, and another 0.65. At the same time I found other mixtures made without agitation, which at first occupied the space of 1.25, were in one case 0.75, another 0.72, and another 0.65.

The reason why I apprehend the diminution goes on so long is, that time is requisite for the action of the phlogiston in the nitrous air upon the dephlogisticated part of the atmospheric air, in order to the conversion of the whole of it into nitrous acid, in consequence of this part being intimately diffused through the phlogisticated part, by which it is, as it were, protected from its action, which is similar to many other chemical processes. It is for the same reason that the diminution is much greater with agitation than without it, as the parts disposed to unite are thereby brought into better contact.

When atmospheric air is exploded together with inflammable air, the diminution never proceeds so far as when nitrous air is mixed with it; because in this case phlogisticated air, as well

well as nitrous acid, is formed by their union; and, as I have shewn, the greater is the proportion of the inflammable air employed, the greater will be the proportion of phlogisticated air in the residuum. This mixture, however, will go on diminishing for some time, though not so far as that with the nitrous air; because part of this produce being nitrous acid, as I have shewn in a former course of experiments, it will require time to be formed, as well as when the nitrous air is employed.

Having made a mixture of equal parts of inflammable and atmospherical air, and exploded them on the 3d of August, I observed that it then occupied the space of 1.35 measures, and on the 2d of September, when I perceived that the diminution would proceed no farther, it was 1.14 which, though considerable, was far short of the diminution produced by an equal bulk of nitrous air.

Though, in the experiments recited above with the *calcined bones*, and the *steel*, neither of these substances appeared to have lost any weight that I was able to ascertain, it does not follow that nothing was emitted from them. For *light* and *heat* are almost universally allowed to be *substances*, though no person has been able to weigh them. Besides the quantity of the materials that I made use of might be too small for the purpose. What

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is most important in the experiments is that, since the diminution of the air was effected by heating those substances, and they did not *gain* any weight in the process, the phlogification of air is not the absorption of any part of it by the substance which produces that effect, as the antiphlogistic theory supposes.

B**FARTHER**

FARTHER EXPERIMENTS
RELATING TO THE
GENERATION OF AIR FROM WATER.

IN a late publication, containing an account of some experiments relating to *the generation of air from water**, I mentioned three different processes in which air was produced from the same water, without any perceivable limit.

The first process was converting the whole of a quantity of water into steam, in the common method of boiling; when I found that, though the water had been boiled ever so long, or ever so often, air continued to be produced from it.

In order to obviate the objection to the water having imbibed the air from the atmosphere, in a second process I put the water on which I operated into long glass tubes, over a column of mercury; and after producing air by keeping the upper part of the tube containing the water a long time in the form of vapour, I let out the air so procured under mercury, by which means the water never came into any contact with the air of the atmosphere, and yet it continued to yield air whenever the process was repeated, without any perceivable diminution, or limit.

* Read before the Philosophical Society of London, and published separately in 1793. E.

In the third process, no heat was used, but the water was put into a glass vessel consisting of a large bulb, connected with a tube the full length of a barometer, a quantity of mercury sufficient to fill the tube being put into it along with the water, and then inverted, and placed in a basin of mercury. By this means the pressure of the atmosphere was removed from the water, and thus the air naturally contained in it escaped, and lodged on the surface of the water; and by inverting the vessel again, it was thrown out into the open air. This process I kept repeating with the same water more than a year, and yet, as in the former processes, I found fresh air always produced from it, and seemingly in an equable manner.

It has been said that, in this process, the water, deprived of all air, instantly seizes upon some the moment that the newly extricated air is thrown out, the surface of the water in the tube being then, though but for a moment, exposed to the atmosphere. But this supposed eager attraction of air by the water would have made it to absorb the newly produced air, if not in its rarefied state on the surface of the water, yet when it was condensed, on inverting the tube, during the time that it was passing the whole length of the tube, as readily as fresh air from the atmosphere. Besides, it requires a considerable time before the water thus deprived of all air will absorb that

which has been produced, or extricated from it, when the vessel is inclined, and consequently the pressure of the atmosphere removed.

Also, in order to obviate this objection, I kept the extremity of the tube carefully covered with my finger all the time that it was inverted till the moment that the air must be let out, and mercury put in, so that it was not exposed to the atmosphere so much as the tenth of a second; and yet I found repeatedly, that the air was produced as readily as when it had been exposed to the atmosphere (as I sometimes purposely did it) several minutes.

I would farther observe, that in this process, if the vessel containing the water and mercury be inverted, and a vacuum appear, as it instantly will, in the form of a bubble, for ever so short a time, a perceivable bubble of permanent air will be produced. I do not therefore see but that, by means probably of heat, air is producible from the same water without any limit.

In order to make any quantity of water as free from air as possible, *agitation* is necessary. But when by the frequent repetition of this process the greatest effect has been produced, and the air, or vapour, has remained long upon the water, agitation will diminish it, part of the newly generated air being imperfectly formed, and more readily imbibed by the water than that

that which had been a longer time in the state of air. This diminution of the bulk of a bubble of air by agitation appears to be the most certain test of as perfect an extrication of air from water as can be attained. But even after this, whenever the bubble of air is let out, and the vessel is inverted, another bubble is instantly formed, sometimes indeed so small as not to be visible to the naked eye, but always by means of a magnifying glass, and this very small quantity will not be absorbed by the water till the vessel has been laid in an inclined position some hours. If the vessel be placed perpendicularly, the bubble will come to be of a considerable size. Still however it will not increase beyond a certain quantity, though it remain in that position ever so long.

I have tried every method that I could think of to deprive water of its power of producing air, but without effect. Heat I found of no use but to assist in expelling the air originally contained in it, and freezing had no more effect than heat.

When I published the pamphlet above mentioned I had not procured from water any other kind of air than such as was, in a greater or less degree, purer than that of the atmosphere, and therefore I imagined that this might have been the origin of all the air in the atmosphere.

But I have since found that though the first quantity of air that is expelled from water is much purer than that of the atmosphere, the next is less pure, and at last it is wholly phlogisticated. This I could not discover while I made use of small bulbs; but when I used large ones, containing from fifty to an hundred ounces of water, it was ascertained with the greatest certainty. From this fact it may be inferred, either that the air produced from water is not that which had been imbibed from the atmosphere, or that, though it imbibes most readily that which is the purest, it retains with the greatest obstinacy that which is least pure, which is analogous to other chemical affinities. If the air thus produced be really generated from the water, or rather vapour, it must be wholly phlogisticated, and afterwards purified by the process of vegetation; or the phlogisticated part alone of the atmosphere may have had that origin, and the dephlogisticated part have come from vegetation.

I once thought that a very small quantity of any of the *acids* enabled water to yield more air than it would do without them, and while I used only small bulbs, I continued to think so; but when I used the larger vessels above mentioned, I could not perceive any sensible difference in the results in consequence of this circumstance.

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The quantity of air extricated from distilled water before the production becomes equable is about one fortieth of its bulk.

Wishing to leave nothing undone that I was capable of doing with respect to this course of experiments, I have, since the publication of the tract above mentioned, endeavoured to convert the whole of a small quantity of water into air, but it has been without effect. Having provided barometer tubes with bulbs connected with them, from one inch to three inches in diameter, I first put into them a small quantity of water, and then filling them with mercury, left them some time with the orifices of the tubes upwards, in order to give the water an opportunity to rise to the top. I then inverted them, when after some time a very small quantity of water would be visible on the surface of the mercury in the tube, and the vapour arising from it in *vacuo* would, of course, be diffused through the whole of the bulb above it. After this, inclining the vessel, and making it lean over a fire, that small quantity of water was wholly converted into vapour, so as to cause the mercury to descend, and leave both the whole of the bulb, and part of the tube, filled with hot vapour, and in this state I kept it several hours. After this I always found a quantity of air produced, and this I threw out,

by inverting the vessel. Then exposing it again to the heat, I never failed to get more air; and having done this, in some cases, not less than twenty or thirty times, I was satisfied that even the smallest quantity of water will never cease to yield air, and in several cases I have by this means procured more air than the bulk of the water.

As some water would necessarily insinuate itself between the mercury and the glass, I exposed almost the whole of the tube containing the mercury to the heat; by this means converting that water into vapour, and making it ascend to the top of the mercury; then throwing out both the water and the air produced from it again and again, I at length found nothing but air above the mercury. Still, however, the whole of the water was not converted into air. For when, by means of heat, the mercury was made to descend, the water which had been confined between the mercury and the glass made its appearance, though by the ascent of the mercury it would again disappear.

I have also found that when there was any sensible quantity of water above the mercury, and have exposed it to heat day after day, the quantity of air, in this case as well as the preceding without heat, came to a *maximum*, and

no repetitions of the process would increase it. This induces me to conclude that the longest continuance of any quantity of water in the state of vapour would not convert it into air. It may, however, be worth while, if there should be an opportunity of doing it without much expence, to make the experiment.

The purest distilled water should be used in these experiments. Instead of this, I once used pump water; but found that, after the production of air was advanced to its maximum, it began to yield a considerable quantity, at least ten times more than it had done before, at the same time becoming a little turbid. But when it was clear, it still yielded much more air than distilled water. Probably some calcareous matter dissolved in the water was decomposed in this process, and the air contained in it had increased the bulk of that which had been produced by means of the water.

Having, in the manner above mentioned, found an easy method of expelling from a quantity of water all the air contained in it, I wished to know what would be the result of making it imbibe different kinds, and various mixtures of air. I had before found that water deprived of its air by boiling would imbibe any kind of air, and that when this air was again expelled by heat, the quality of it was not changed; but

I could

I could now both expel the air more effectually, and make it imbibe any particular kind of air with more certainty and expedition. For this purpose, having first expelled the air, by removing the pressure of the atmosphere in the manner described above, I inclined the vessel, laying it in a position nearly horizontal, with the end of the tube immersed in a basin of mercury; and then having introduced the air that I wished it to imbibe, I gently agitated the vessel, and the pressure of the atmosphere being now removed, the water would pretty soon saturate itself with the air. After this, the vessel being placed upright, the air which it had imbibed was presently discharged, without any application of heat.

In this method, beginning with atmospherical air, which consists of a mixture of dephlogisticated and phlogisticated air, I found that water imbibes the former in preference to the latter, but not wholly unmixed with it. Having made 45 ounces of distilled water free from air, I put to it $2\frac{1}{4}$ ounce measures of atmospherical air, of which, by agitation, it imbibed three fourths of a measure, when the remaining two ounce measures were found to be of the standard of 1.15 instead of 1.01 which was the standard of the air before the process; that is, when one measure of this air was
mixed

mixed with one measure of nitrous air, it occupied that space. When the air that had been imbibed was expelled from the water, it was of the standard of 0.75. Both mixed together were exactly of the standard of atmospherical air.

I had thought that, though dephlogisticated and inflammable air will not unite while they retain their aërial form, without a red heat, they might do so when they were both deprived of that form, by being combined with water, and make phlogisticated air; I therefore made a quantity of water deprived of all air imbibe a mixture of equal quantities of those two kinds of air. But when this mixed air was expelled from the water, it was fired with an explosion, so that no union had been formed between them. I then made a quantity of water imbibe the two kinds of air one after the other, but there was no difference in the result. The air that was expelled from the water was still fired with one explosion.

But dephlogisticated and nitrous air, which unite without heat in their aërial form, did the same when they were combined with water. Having expelled all its air from a large quantity of distilled water, I first made it imbibe as much as it could of nitrous air, and after that of dephlogisticated air, and observed that what remained of each, not absorbed by the water, was very little changed.

changed. Then, expelling the air from the water thus doubly impregnated, the first quantity procured was dephlogisticated, though not so pure as before; the standard of the process with two equal quantities of nitrous air being 0.6, whereas before it had been 0.2. The standard of the second expulsion of air was 0.4. Afterwards it was 0.8, then 1.0; and thus it would, no doubt, have proceeded, till it had been wholly phlogisticated; but no part of it had the property of nitrous air. This kind of air that had been imbibed must have united with as much of the dephlogisticated air contained in the water as it could saturate, and thus have formed nitrous acid, which remained in the water, while the superfluous dephlogisticated air had been expelled in the process.

I then first saturated the water with the dephlogisticated air, and after that with the nitrous air, which it imbibed very readily; and expelling the air afterwards, found it to be purely nitrous, there having been more nitrous air employed at this time than was sufficient to saturate the dephlogisticated air.

Having made the preceding experiments with water, I wished to extend them to other liquid substances, and began with *spirit of wine*, which I had before found to be convertible into inflammable air by a red heat, and also by

by the electric spark. I now find that so great a degree of heat is by no means necessary for this purpose.

If I fill one of the bulbs above mentioned with the spirit, and by means of a column of mercury take off the pressure of the atmosphere, a very great quantity of inflammable air is immediately discharged from it, and by a repetition of the process a smaller quantity never fails to be produced, and as far as I see without limits.

If in this state I expose the spirit to a degree of heat sufficient to convert it into vapour, a very great proportion of it is presently converted into air, and in a few minutes the quantity produced will be ten or twenty times the bulk of the liquid. This is the case repeatedly with the same spirit, so that I have no doubt but that, in time, the whole of it would be so converted, just as if it had been exposed to a red heat in passing in the form of vapour through a red hot earthen tube.

Having expelled a very great quantity of air from one of the bulbs filled with spirit of wine, of the specific gravity of 682.5, I exposed it to the atmosphere, after which it yielded as much as before, viz. about one third or one fourth of its bulk, all strongly inflammable. I had the
same

same result in the subsequent process. After another, the air was exploded like a mixture of inflammable and atmospherical air, and the next produce burned with a lambent flame. Being then examined, its specific gravity was 692.4; so that it had acquired some weight by imbibing atmospherical air.

Having, in like manner, expelled air which I found to be inflammable from a quantity of *spirit of turpentine*, I made it imbibe atmospherical air, and expelling it again, found it to be not inflammable, but phlogisticated. This fluid had also gained something in its specific gravity by the process.

The only objection that, after giving much attention to the subject, I think can be made to the conclusion that I first drew from these experiments, viz. that air is actually produced from water, is the very small quantity that is produced in proportion to the bulk of the water, after the air naturally contained in it is wholly expelled. But if it shall appear, after a long course of time, that this small production of air from the same water is constant and equable, I do not see how the conclusion, extraordinary as it may be thought, can be disputed. This air being wholly *phlogisticated* is a sufficient proof that the air so produced is not

absorbed from the atmosphere in the course of the process. For then it would have been dephlogisticated, or at least purer than that of the atmosphere, which water always seizes upon in preference to that which is impure.

THE END.

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CONSIDERATIONS
ON THE
DOCTRINE OF PHLOGISTON,
AND THE
DECOMPOSITION OF WATER.

By JOSEPH PRIESTLEY, L. L. D. F. R. S. &c. &c.

Qualem commendat etiam atque etiam aspice.

HORACE.

PHILADELPHIA, PRINTED.

LONDON:

REPRINTED BY J. JOHNSON, IN ST. PAUL'S CHURCH-YARD.

1796.

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1793

To Messrs. Berthollet, De la Place, Monge,
Morveau, Fourcroy, and Hassenfratz,

The surviving Answerers of Mr. Kirwan.

GENTLEMEN,

HAVING drawn up a short defence of the doctrine of *phlogiston*, I take the liberty of inscribing it to you, as the principal advocates for the Antiphlogistic theory. My view in this is to draw your attention once more to the subject, and I request the favour of an answer to my objections. I hope I am not wanting in a proper deference to the opinion of men so justly eminent as yourselves and your friends in France, and also that of great numbers in England, and wherever chemistry is known, who have adopted your hypothesis. But you will agree with me, that no man ought to surrender his own judgment to any mere *authority*, however respectable. Otherwise, your own system would never have been advanced.

As you would not, I am persuaded, have your reign to resemble that of *Robespierre*, few as we are who remain disaffected, we hope you had rather gain us by persuasion, than silence us by power. And though we are all apt to flatter ourselves,

ourselves, we hope we are willing to be influenced by the former, as we are inflexible to the latter. If you gain as much by your answer to me, as you did by that to Mr. Kirwan, your power will be universally established, and there will be no *Vendée* in your dominions.

Differing as we do in this respect, we all agree in our wishes for the prevalence of *truth*, and also of *peace*, which is wanted as much for the interest of philosophy, as those of humanity.

I subscribe myself,

Your fellow-citizen,

JOSEPH PRIESTLEY,

Northumberland, in America,
June 15, 1796.

CONSIDERATIONS
ON THE
DOCTRINE OF PHLOGISTON,
AND THE
DECOMPOSITION OF WATER.

THE INTRODUCTION.

THERE have been few, if any, revolutions in science so great, so sudden, and so general, as the prevalence of what is now usually termed *the new system of chemistry*, or that of the *Anti-phlogistians*, over the doctrine of Stahl, which was at one time thought to have been the greatest discovery that had ever been made in the science. I remember hearing Mr. Peter Wolfe, whose knowledge of chemistry will not be questioned, say, that there had hardly been any thing that deserved to be called a *discovery* subsequent to it. Though there had been some who occasionally expressed doubts of the existence of such a principle as that of *phlogiston*, nothing had been advanced that could have laid the foundation of *another system* before the labours of Mr. Lavoisier and his friends, from whom this new system is often called that of the *French*.

This system had hardly been published in France, before the principal philosophers and chemists of England, notwithstanding the rivalry which has long subsisted between the two countries, eagerly adopted it. Dr. Black in Edingburgh, and as far as I hear all the Scots have declared themselves converts, and what is more, the same has been done by Mr. Kirwan, who wrote a pretty large treatise in opposition to it. The English reviewers of books, I perceive, universally favour the new doctrine. In America also, I hear of nothing else. It is taught, I believe, in all the schools on this continent, and the old system is entirely exploded. And now that Dr. Crawford is dead, I hardly know of any person, except my friends of the Lunar Society at Birmingham, who adhere to the doctrine of phlogiston; and what may now be the case with *them*, in this age of revolutions, philosophical as well as civil, I will not at this distance answer for.

It is no doubt *time*, and of course opportunity of examination, and discussion, that gives stability to any principles. But this new theory has not only kept its ground, but has been constantly and uniformly advancing in reputation, more than *ten years*, which, as the attention of so many persons, the best judges of every thing relating to the subject has been unremittingly given to it, is no inconsiderable period.

period. Every year of the last twenty or thirty has been of more importance to science, and especially to chemistry, than any ten in the preceding century. So firmly established has this new theory been considered, that a *new nomenclature*, entirely founded upon it, has been invented, and is now almost in universal use; so that, whether we adopt the new system or not, we are under the necessity of learning the new language, if we would understand some of the most valuable of modern publications.

In this state of things, an advocate for the old system has but little prospect of obtaining a patient hearing. And yet, not having seen sufficient reason to change my opinion, and knowing that free discussion must always be favourable to the cause of truth, I wish to make one appeal more to the philosophical world on the subject, though I have nothing materially new to advance. For I cannot help thinking that what I have observed in several of my publications has not been duly attended to, or well understood. I shall therefore endeavour to bring into one view what appears to me to be of the greatest weight, avoiding all extraneous and unimportant matter; and perhaps it may be the means of bringing out something more decisive in point of *fact*, or of *argument*, than has hitherto appeared.

No person acquainted with my philosophical publications can say that I appear to have been particularly attached to any hypothesis, as I have frequently avowed a change of opinion, and have more than once expressed an inclination for the new theory, especially that very important part of it *the decomposition of water*, for which I was an advocate when I published the sixth volume of my experiments; though farther reflection on the subject has led me to revert to the creed of the school in which I was educated, if in this respect I can be said to have been educated in any school. However, whether this new theory shall appear to be well founded or not, the advancing of it will always be considered as having been of great importance in chemistry, from the attention which it has excited, and the many new experiments which it has occasioned, owing to the just celebrity of its patrons and admirers.

SECTION I.

OF THE CONSTITUTION OF METALS.

ACCORDING to the doctrine of phlogiston, advanced by Becher and Stahl in the beginning of this century, and much simplified and improved since their time, metals, phosphorus, sulphur, and many other substances which are supposed to contain it, are compounds, consisting of this principle, and another which may be called its *base*. Thus each of the metals contains phlogiston united to a peculiar calx, and sulphur and phosphorus consist of the same principle and their respective acid, or the bases of them. But according to the antiphlogistic theory, all the metals are simple substances, and become calces by imbibing pure air; and sulphur and phosphorus are also simple substances, and become the acids of vitriol and of phosphorus by imbibing the same principle, called by them *oxygen*, or the principle, as it probably is, of universal acidity.

As a proof that metals are simple substances, and that they become calces merely by imbibing air, they allege the case of mercury, which becomes the calx called *precipitate per se* by exposure to the atmosphere in a certain degree of heat, and which becomes run-

ning mercury again by exposure to a greater degree of heat. They therefore think it impossible not to conclude, that in all other cases of calcination, as well as this, the only difference between the calx and the metal, is that the latter has parted with the air which it had imbibed.

But this is the case of only this particular calx of this metal, and there is another calx of the same metal, viz. that which remains after exposing turbith mineral to a red heat, which cannot be completely revived by any degree of heat, but may be revived in inflammable air, which it imbibes, or when mixed with charcoal, iron-filings, or other substances supposed to contain phlogiston. And if this calx of mercury, or (supposing it to contain some acid of vitriol) this salt, necessarily requires some addition to constitute it a metal, all mercury must contain the same. For though with the same external appearance, the same metal may contain different proportions of any particular principle, as phlogiston, they must be denominated different substances, if some specimens contain this element, and others be wholly destitute of it. All, therefore, that can be inferred from the experiment with the precipitate per se is, that in this particular case, the mercury in becoming that calx imbibed air, without parting with any, or very little of its

its phlogiston; and if we judge by the air expelled from the calces of metals and other circumstances, there are few, if any, of them but contain more or less of phlogiston.

I would observe in this place, that it is asserted by some very able chemists, that if the precipitate per se be made with proper attention, it will be revived without yielding any air. This is also the case with *minium* when fresh made. But this is owing, I doubt not, to their wanting *water*, which I deem to be essential to the constitution of every kind of air; so that they both contain the element of dephlogisticated air, though, for want of water, it is not able to assume that form.

That mercury may have the same external appearance, and all its essential properties, and yet contain different proportions of something that enters into it, is evident from the phenomena of its solution in the nitrous acid, and the revival of its calx in inflammable air. According to the old theory, there is a loss of some part of its phlogiston in the solution of mercury in the nitrous acid, since nitrous air is procured in the process. And though it may be revived from its precipitates by mere heat, yet if it be revived in a vessel of inflammable air, it will imbibe it in great quantities. Mercury revived in these circumstances must contain more phlogiston than that which is revived from

from the same calx by mere heat. But though mercury revived by mere heat after a solution in nitrous acid must have a deficiency of phlogiston, and when it is revived from precipitate per se inflammable air must contain a redundancy of the same principle, yet there will hardly be a doubt but that, in all chemical processes, it would exhibit the same phenomena.

In all other cases of the calcination of metals in air, which I have called the *phlogification* of the air, it is not only evident that they gain something, which adds to their weight, but that they likewise part with something. The most simple of these processes is the exposing iron to the heat of a burning lens in confined air, in consequence of which the air is diminished, and the iron becomes a calx. But that there is something emitted from the iron in this process is evident from the strong *smell* which arises from it. If the process be continued, inflammable air will be produced, if there be any moisture at hand to form the basis of it. From this it is at least probable, that, as the process went on in an uniform manner, the same substance, viz. the basis of inflammable air, was continually issuing from it; and this is the substance, or principle, to which we give the name of *phlogiston*.

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That the effect of this process is not, as the antiphlogistians assert, the mere separation of the dephlogisticated from the phlogisticated air in that of the atmosphere, I have proved in a course of experiments, in which I have shewn that a considerable part of the phlogisticated air that is found after this process is formed in the course of it, by the union of the phlogiston from the iron with the dephlogisticated air. And if the calcination of the iron in this process be always attended with the loss of some constituent part of it, the same is, no doubt, the case with all other calcinations of the same metal, and also those of all other metals. And farther, if the *metals* be compound substances, containing phlogiston united to some base, the same is the case with *sulphur* and *phosphorus*, because they become acids when they are used in the same process.

According to the antiphlogistic theory, the inflammable air that is produced in the solution of metals in any acid comes wholly from the water combined with it, and not at all from the metal dissolved. But the advocates for this theory do not seem to have attended to one necessary consequence of this supposition. According to their own principles, water consists of eighty-seven parts of oxygen, to only thirteen of hydrogen, in every hundred, which is nearly seven times as much of the former as
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of the latter. Consequently, since nothing but hydrogen escapes in the process, there must remain, from this decomposition of the water, seven times as much oxygen in the solution. But both Mr. Lavoisier and Mr. de la Place say (*Examination of Mr. Kirwan's Treatise*, p. 197, 198), what I doubt not is strictly true, that after the process the acid will saturate exactly the same quantity (they do not say more) of alkali, that it would have done before; whereas, with the addition of so much oxygen, it ought to saturate considerably more. If the oxygen from the decomposition of the water do not join that in the acid, what becomes of it?

If this case be analogous to that of the supposed decomposition of water by hot iron, the oxygen ought to be lodged in the iron, and compose finery cinder. But this substance is not soluble in vitriolic acid, if that be employed in the experiment; and when it is dissolved in the marine acid, it does not dephlogisticate it, as minium, and other substances containing oxygen, do. It is evident, therefore, that there is no addition of oxygen in this process, consequently no decomposition of water in the case, and that the inflammable air must come from the decomposition of the iron.

SECTION II.
OF THE COMPOSITION AND DECOMPOSITION
OF WATER.

THE antiphlogistic theory has received its greatest support from the supposed discovery that water is resolvable into two principles, one that of *oxygen*, the base of dephlogisticated air, and the other, because it has no other origin than water, *hydrogen*, or that which, with the addition of *calorique*, or the element of *heat*, constitutes inflammable air. " One of the " parts of the modern doctrine the most solidly " established, say Mr. Berthollet, and the other " authors of the *Report* on this subject (*Examination of Kirwan*, p. 17), is the formation, " the decomposition, and recomposition, of water. And how can we doubt of it, when " we see that, in burning together fifteen " grains of inflammable air, and eighty-five " of vital air, we obtain exactly an hundred " grains of water, in which, by decomposition, " we find again the same principles, and in " the same proportions. If we doubt of a " truth established by experiments so simple, " and palpable, there would be nothing certain in natural philosophy. We might even " question whether vitriolated tartar be composed

“ posed of vitriolic acid and fixed alkali, or sal
“ ammoniac of the marine acid and volatile al-
“ kali, &c. &c. For the proofs that we have
“ of the composition of these salts are of the
“ same kind, and not more rigorous, than those
“ which establish the composition of water. No-
“ thing, perhaps, more clearly proves the weak-
“ ness of the old theory, than the forced ex-
“ planations that have been attempted to be
“ given of these experiments.”

Notwithstanding the confidence thus strongly expressed by these able and experienced chemists, I must take the liberty to say, that the experiments to which they allude appear to me to be very liable to exception, and that the doctrine of phlogiston easily accounts for all that they observed.

Their proof that water is decomposed, and resolved into two kinds of air, is that when steam is made to pass over red-hot iron inflammable air is produced, and the iron acquires an addition of weight, becoming what is called *finery cinder*; but what they call *oxide of iron*, supposing that there is lodged in it the oxygen which was one of the constituent parts of the water expended in the process, while the other part, or the hydrogen, with the addition of heat, assumed the form of inflammable air.

But in order to prove that this addition of weight to the iron is really oxygen, they ought to be
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be able to exhibit it in the form of dephlogisticated air, or of some other substance into which oxygen is allowed to enter, and this they have not done. Iron that has really imbibed air, or the common *rust of iron*, has a very different appearance from this finery cinder, being *red*, and not *black*; and when treated in similar processes, exhibits very different results. Mr. Fourcroy says, (Ib. p. 251), that this finery cinder is "iron partially oxygenated." But if that were the case, it would go on to attract more oxygen, and in time become a proper rust of iron, completely oxygenated. But this is so far from being the case, that finery cinder never will acquire rust; which shews that the iron in this state is saturated with some very different principle, which even excludes that which would have converted it into rust.

However, neither this, nor any other calx of iron, can be revived unless it be heated in inflammable air, which it eagerly imbibes, or in contact with some other substance which has been supposed to contain phlogiston. The probability therefore is, that the phlogiston then enters this calx of iron, replacing that which had been expelled to form the inflammable air. Nor can any inflammable air be procured in this process with steam, but by means of some substance which has been supposed to contain phlogiston. Where then, is

the certain proof that water is decomposed in this process?

It may be said that the oxygen imbibed by this iron, being expelled by heat in contact with inflammable air, unites with that air, and with it constitutes the water which is found after the process. But for any thing that appears, this water may be that which the iron had imbibed, and which can only be expelled from it by the entrance of that phlogiston which it had lost.

This is the more probable, since, when any other substance which is certainly known to contain oxygen, is heated in the same circumstances, *fixed air* (which is allowed to contain oxygen) is found, and this is not the case with this calx of iron. If, for example, precipitate per se, or minium, be heated in inflammable air, the mercury and the lead will be revived, and a considerable quantity of fixed air will be produced at the same time. But if the air be previously expelled from the minium, which converts it into a yellow substance called *massicot*, though the lead will be revived, no fixed air will be generated. Since, therefore, the result of treating finery cinder and massicot is precisely the same, in the same circumstances, we are fully authorised to conclude that the substances themselves are similar, and consequently that the
finery

finery cinder contains no more oxygen than mafficot.

In another important respect finery cinder and mafficot are similar. They are both soluble in marine acid without dephlogisticating it, which minium instantly does. And yet Mr. Berthollet says, *Annales-de Chymie*. Vol. 3. p. 96, that "the heat by which minium becomes "mafficot cannot change its nature." What is the evidence of a change in the *nature* of any thing, but a change of its *properties*? On the whole, therefore, the probability is, that when iron is converted into finery cinder, it loses its phlogiston, and imbibes only water; and that when it is reconverted into iron, it parts with the water, and recovers its phlogiston. N. B. The experiment with the mafficot must be tried presently after it is made, since it will very soon imbibe air from the atmosphere.

In this place I would observe that, if it be admitted that there is a principle in inflammable air, which, being imbibed by the calx of a metal, converts it into a metallic substance, it will follow that the same principle is contained in charcoal, and other combustible substances; because they will all produce the same effect, and therefore that the principle of inflammability, or phlogiston, is the same in them all.

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Another pretended proof that water is composed of dephlogisticated and inflammable air, is that when the latter is burned slowly in the former, they both disappear, and a quantity of water is produced, equal to their weight. I do not, however, find that it was in more than a single experiment that water so produced is said to have been entirely free from acidity, though this experiment was on a large scale, not less than twelve ounces of water being procured. But the apparatus employed does not appear to me to admit of so much accuracy as the conclusion requires; and there is too much of correction, allowance, and computation, in deducing the result. Also it is, after all, acknowledged that, after decomposing this quantity of the two kinds of air, and making all the allowance they could for the phlogisticated air, or *azote*, in the dephlogisticated air, they found fifty-one cubic inches of this kind of air more than they could well account for. This quantity, therefore, and perhaps something more (since the operators were interested to make it as small as possible) must have been formed in the process. And when this kind of air, as well as inflammable, is decomposed together with dephlogisticated air, nitrous acid is produced. The probability therefore is, that the acidifying principle, or oxygen, in the dephlogisticated air which they decomposed, was contained in that phlo-

phlogisticated air, and that, had the process been conducted in any other manner, it would have assumed the form of nitrous acid. They acknowledge that, except when the inflammable air was burned *in the slowest manner*, the water they produced had more or less of acidity.

The experiments which I made on the decomposition of these two kinds of air in *close vessels*, appear to me to be much less liable to exception, and the conclusion drawn from them is the reverse of that of the French philosophers.

When dephlogisticated and inflammable air, in the proportion of a little more than one measure of the former to two of the latter, both so pure as to contain no sensible quantity of phlogisticated air, are inclosed in a glass or copper vessel, and decomposed by taking an electric spark in it, a highly phlogisticated nitrous acid is instantly produced; and the purer the airs are, the stronger is the acid found to be. If phlogisticated air be purposely introduced into this mixture of dephlogisticated and inflammable air, it is not affected by the process, though, when there is a considerable deficiency of inflammable air, the dephlogisticated air, for want of it, will unite with the phlogisticated air, and, as in Mr. Cavendish's experiment, form the same acid. But since both the kinds of air, viz. the inflammable and

the phlogificated, contribute to form the same acid, they must contain the same principle, viz. phlogiston.

If there be a redundancy of inflammable air in this process, no acid will be produced, as in the great experiment of the French chemists, but in the place of it there will be a quantity of phlogificated air. A considerable quantity of *water* is always produced in these decompositions of air. But this circumstance only proves that the greatest part of the *weight* of all kinds of air is water. I have, in my experiments on *terra ponderosa aerata* demonstrated that water constitutes about half the weight of fixed air.

The reason, no doubt, why, in the experiments of the French chemists, the water they produced was not without acidity, whenever the flame they made use of was too strong, was that, in that case, more of the dephlogificated air in proportion to the inflammable was consumed, than when the flame was weak; so that the results of their experiments exactly coincide with those of mine.

When the decomposition of dephlogificated and inflammable air is made in a glass vessel, a peculiarly *dense vapour* is formed, which the eye can easily distinguish not to be mere vapour of *water*, and if the juice of turnsole be put into the vessel, it immediately becomes of
4 a deep

a deep red, which shews that it was an acid vapour.

Since the acid that I procured in this process was in considerable quantity, and no phlogisticated air was present (for in the last of my experiments I did not even make use of an air-pump, but first filled the vessel with water, and then displaced it by the mixture of the airs), I do not see how it is possible to account for the formation of this acid but from the union of the two kinds of air; and it can hardly be supposed that, in the very same process, the decomposition of the same substances should compose others so very different from each other as *water* and *spirit of nitre*. I think I have sufficiently accounted for the result of the experiments made by the French chemists on the common hypothesis, which supposes inflammable air to contain phlogiston, but I do not yet see how it is possible for them to explain mine on theirs, according to which there is no such principle in nature. Upon the whole, it does not appear to me that the evidence either for the composition, or the decomposition of water, is at all satisfactory; and certainly the arguments in support of an hypothesis so extraordinary, and so novel, ought to be of the most conclusive kind.

SECTION III.

OTHER OBJECTIONS TO THE ANTIPHLOGISTIC THEORY.

HAVING considered the evidence that has been alleged in support of the antiphlogistic theory, and found it to be insufficient, I shall, in this section, mention a few objections that may be made to it from other considerations.

1. If inflammable air, or hydrogen, be nothing more than a component part of water, it could never be produced but in circumstances in which either water itself, or something into which water is known to enter, is present. But in my experiments on heating finery cinder together with charcoal, inflammable air is produced, though, according to the new theory, no water is concerned. According to this theory, finery cinder, called the *oxide of iron*, consists of nothing besides iron and oxygen; and the charcoal, made with the greatest degree of heat that can be applied, is equally free from water; and yet when these two substances are mixed together, and exposed to heat, they yield inflammable air in the greatest abundance.

This fact I cannot account for on the principles of the new theory; but nothing is easier on those of the old. For the finery cinder containing

taining water, as one of its component parts, gives it out to any substance from which it can receive phlogiston in return. The water, therefore, from the finery cinder uniting with the charcoal makes the inflammable air, at the same time that part of the phlogiston from the charcoal contributes to revive the iron. Inflammable air of the very same kind is procured when steam is made to pass over red-hot charcoal.

2. Though the new theory discards phlogiston, and in this respect is more simple than the old, it admits another new principle, to which its advocates give the name of *carbone*, which they define to be the same thing with charcoal, free from earth, salts, and all other extraneous substances; and whereas we say that fixed air consists of inflammable air and dephlogisticated air, or oxygen, they say that it consists of this carbone dissolved in dephlogisticated air. See *Examination of Mr. Kirwan*, p. 79. Mr. Lavoisier says, *lb.* p. 63, that "wherever fixed air has been obtained, there is charcoal." They therefore call it the *carbonic acid*.

But in many of my experiments large quantities of fixed air have been procured where neither charcoal, nor any thing containing charcoal, was concerned, or none in quantity sufficient to account for it. When the purest
malleable

malleable iron is heated in dephlogisticated air, or in vitriolic acid air, a considerable quantity of fixed air is formed. It is said that *plumbago* is contained in iron. But it is not found in malleable iron, and least of all in the air that is expelled from it. Fixed air is also produced by reviving minium in inflammable air, and if charcoal of copper be heated in dephlogisticated air, a quantity of fixed air equal to nine-tenths of the dephlogisticated air will be formed. More than thirty ounce measures of the purest fixed air were by this means procured from six grains of this charcoal, which is made by the union of spirit of wine and this metal.

Lastly, fixed air is procured in great abundance in animal respiration. It is true that fixed air is procured by exposing lime-water to atmospheric air, but it is never procured by this means in air confined in any vessel. There must, for this purpose, be an open communication with the atmosphere. But fixed air will be procured in great abundance by breathing air contained in the smallest receiver, and especially if the air be dephlogisticated. It must therefore be formed by phlogiston, or something emitted from the lungs, uniting with the dephlogisticated air which it meets there. It may be said that since we feed in a great measure upon vegetables (and even animal food is originally

nally formed from them) and this principle of *carbone* is found in all vegetables, *this* may be the substance that is exhaled from the lungs. But since, in this process, it forms the same substance that inflammable air from iron does with dephlogisticated air, or oxygen, it must be the same thing with it; and then this *carbone* will only be another name for *phlogiston*.

3. The antiphlogistians always suppose *azote*, or phlogisticated air, to be a simple substance, though I think abundant evidence has been given (and more will be found in my last memoir, printed in the *Transactions of the Philosophical Society at Philadelphia*),* that it is composed of phlogiston and dephlogisticated air.

4. As to the *new nomenclature*, adapted to the new theory, no objection would be made to it, if it were formed, as is pretended, upon a knowledge of the real constitution of natural substances; but we cannot adopt one, the principles of which we conceive not to be sufficiently ascertained. For other objections to this nomenclature, I refer to the Preface to *Mr. Keir's excellent Dictionary of Chemistry*. However, whether we approve of this new language or not, it is now so generally adopted, that we are under a necessity of learning, though not of using it.

On the whole, I cannot help saying, that it appears to me not a little extraordinary, that

* See the preceding article.

a theory so new, and of such importance, overturning every thing that was thought to be the best established in chemistry, should rest on so very narrow and precarious a foundation, the experiments adduced in support of it being not only ambiguous, or explicable on either hypothesis, but exceedingly few. I think I have recited them all, and that on which the greatest stress is laid, viz. that of the formation of water from the decomposition of the two kinds of air, has not been sufficiently repeated. Indeed, it requires so difficult and expensive an apparatus, and so many precautions in the use of it, that the frequent repetition of the experiment cannot be expected; and in these circumstances the practiced experimenter cannot help suspecting the accuracy of the result, and consequently the certainty of the conclusion.

But I check myself. It does not become one of a minority, and especially of so small a minority, to speak or write with confidence; and though I have endeavoured to keep my eyes open, and to be as attentive as I could to every thing that has been done in this business, I may have overlooked some circumstances which have impressed the minds of others, and their sagacity is at least equal to mine.

The phlogistic theory is not without its difficulties. The chief of them is that we are not able to ascertain the *weight* of phlogiston, or indeed
2 that

that of the oxygenous principle. But neither do any of us pretend to have weighed *light*, or the element of *heat*, though we do not doubt but that they are properly *substances*, capable, by their addition, or abstraction, of making great changes in the properties of bodies, and of being transmitted from one substance to another.

N. B. For answers to the objections of Mr. Lavoisier and Mr. Berthollet to some experiments of mine relating to this subject, I refer to the last edition of my *Observations on Air*, Vol. III. p. 554.

THE END.

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